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SECTION OF GEOLOGY AND MINERALOGY

APRIL 7, 1947

DOCTOR H. H. HESS, Associate Professor of Geology, Princeton University, Princeton, N. J.: *Major Structural Trends of the Western North Pacific.* (This lecture was illustrated by lantern slides.)

During the war, a huge number of soundings were obtained by the United States Navy in the Pacific. Many of these data were collected under combat conditions, and many areas were traversed which would not be crossed by ships in peace-time. The data obtained are being compiled at the Hydrographic Office. The first result of this compilation is the new bathymetric chart, *Korea to New Guinea*, including the Philippine Sea (H.O. 5485).*

Within the region of the above chart, the world's most rugged topography is found. There is a difference of more than 40,000 feet between the mountains of the eastern Philippines and the bottom of the great Mindanao Trench. Other great trenches of the circum-Pacific belt lie to the east of Honshu, the Bonins, the Marianas, Yap, and the Palaus. These form a fairly continuous chain and may be extended northward *via* the Kurile and Aleutian Trenches to North America, so that they form a series more than 6,000 miles long. To the west and north of these trenches are found the island arcs and areas characterized

*A manuscript dealing with the area from Korea to New Guinea will be published in Bull. Geol. Soc. Am. For a paper dealing with some peculiar flat-topped peaks in the Pacific Basin, see in Am. J. Sci. 244: 772–791. 1946.

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by andesitic volcanism, while to the south and east lies the gentle topography of the Pacific Basin, an area of basaltic volcanism.

The *Korea to New Guinea* area is probably also the most highly seismic region in the world. Within it, we find extremes of topography, earthquake activity, volcanism, and also gravity anomalies. This wealth of activity and the large scale of the structures makes it an ideal place to study large tectonic features and their causes. The deep trenches are the topographic expression of the dominant structural feature present. All of the other large structural features are related and subordinate to the crustal buckle beneath the trenches. It is thought that the buckling is related to convection currents in the substratum.

The region from New Guinea to New Zealand is in many respects similar to that discussed above. Here, also, there are extremes of topography, seismic activity, and volcanic activity. This region presents some further problems not seen in the former area. Arcs convex towards the continental area (Australia) are found in the New Hebrides, Solomons, and New Britain, which are the only exception to the rule in present-day arcs. The southern end of the westward-facing arcs is joined to the northern end of an eastward facing arc probably by a great zone of shearing and transcurrent faulting which extends from south of the New Hebrides to the northern end of the Tonga arc.

The Pacific Basin proper has a fairly level, gently undulating floor from which rise abruptly hundreds of peaks which form the present atolls, volcanic islands, and submerged ancient islands, the guyots (described elsewhere). Besides these, there are some very large gentle swells, the forms and distribution of which are as yet very imperfectly known. A belt across the North Pacific from the Equator to 30° N is fairly well explored and the data await compilation. The South Pacific basin remains largely unexplored. Through it passes a very large swell, the so-called Albatross Plateau.

SECTION OF BIOLOGY

APRIL 14, 1947

DOCTOR YALE KNEELAND, JR., Department of Medicine, College of Physicians and Surgeons, Columbia University, New York, N. Y.: *The Common Cold.* (This lecture was illustrated by lantern slides).

(No abstract received.)

DOCTOR RAYMOND L. LIBBY, Stamford Research Laboratories, American Cyanamid Company, Stamford, Connecticut: *The Use of Tagged Antigens in Immunochemical Studies.** (This lecture was illustrated by lantern slides.)

There are a number of problems to be considered before embarking on a program of biological research involving radio-tracers. The first is what radioactive isotope to use. For biological work, the most desirable radio isotopes would be oxygen, nitrogen, carbon, and hydrogen. Of these four elements, there are no suitable radio isotopes available for oxygen or nitrogen. Carbon has two isotopes, C^{11}, with a half-life of 21 minutes, and C^{14} with a half-life of over 1,000 years. Hydrogen has one radioactive isotope, H^3, with a half-life of 20 years. There are a number of difficulties in using these three isotopes: C^{11}, because of its short half-life, is limited in its usefulness, while C^{14} and H^3 are limited in their usefulness because of the difficulties in measuring their soft radiation.

Among the other elements that might be used in biological studies, there are a number of very useful radioactive isotopes, such as P^{32} with a half-life of 14.3 days, S^{35} with a half-life of 88 days, I^{131} with a half-life of 8 days, Ca^{45} with a half-life of 180 days, Fe^{59} with a half-life of 47 days, and Na^{24} with a half-life of 14.8 hours.

A second problem to be considered is the synthesis of the compound to be studied. In our case, we are interested in tagged antigens or proteins, and, since these cannot be synthesized directly in a test tube, we are forced to adopt indirect methods through the mediation of the living cell.

A number of workers[1–4] have demonstrated that the nucleic-acid portion of the nucleoprotein molecule can be tagged by injecting or feeding animals with sodium phosphate in which the phosphorus is radioactive. Bacterial proteins containing phosphorus may be tagged by growing the organisms in a nutrient solution containing $Na_2HP^*O_4$. The method we have used to tag the tobacco mosaic virus (TMV) molecule will be given in detail subsequently. There are a number of ways in which proteins may be tagged directly. For example, radioactive Arsenilic-azo protein could be prepared by using radioactive arsenic, or radioactive iodinated proteins may be prepared by using radioactive iodine.

* This address was delivered before the meeting of the Section of Biology on February 10, 1947.

The third problem that must be considered is the specific activity. This refers to the ratio of the radioactive atoms to stable atoms in the compound. In most biological work, it is desirable that the specific activity be relatively high in order to permit one to follow the compound through several half-lives and to overcome the relatively large dilution factors involved in most of this work.

The fourth problem to be considered is that of exchange between stable elements and radioactive elements. It is impossible to interpret the data obtained in certain problems unless the possibilities of exchange are known. For example, if one were to study the distribution of radioactive iodoglobulin in the body, it would be essential to know the possibilities of exchange between the tagged iodoglobulin and sodium iodide. *Vice versa*, one should know the possibilities of exchange between radioactive sodium iodide and untagged iodoglobulin.

The work to be reported on was undertaken in an effort to explore the usefulness of radio tracers in determining the distribution and the rate of breakdown of injected antigens. More specifically, we set out to try to answer the question of whether or not antigen must be present for the formation of antibody. Among the various theories of antibody formation, two are especially notable here. One postulates that antigen acts as a template and thus must be present for the formation of antibody, while the second postulates that the antigen acts to modify some enzyme, which in turn is involved in the synthesis of antibody globulin, and that even after the antigen is broken down more antibody may be synthesized.

For the present study, it was decided to attempt to tag tobacco mosaic virus. This virus was chosen for a number of reasons. First, it was a nucleo-protein and thus contained phosphorus; second, it could be obtained in reasonable quantities by extraction from infected tobacco plants; third, methods were available which would permit the preparation of highly purified solutions; and, fourth, tobacco mosaic virus had been reported in the literature to be an excellent antigen.

The TMV was tagged by inoculating seedling Turkish tobacco plants with the virus. These plants were then transferred to individual bottles containing a nutrient solution composed of KNO_3, $MgSO_4$, $CaCl_2$, and the necessary trace elements. Sodium phosphate containing 278 microcuries of radio P was then added to each bottle. The specific activity of the phosphorus in the $Na_2HP^*O_4$ was 70 microcuries per mg. of phosphorus. The level of the radio activity in the nutrient solution

was maintained by weekly additions of $Na_2HP^*O_4$ or a complete change of the nutrient solution.

After thirty days, the stems and leaves of the tobacco plants were removed, macerated, and the TMV extracted and recrystallized three times. It was found that this purified preparation contained considerable radioactivity. From the radioactivity found per mg. of TMV and the reported percentage of phosphorus in TMV, the specific activity was calculated to be 7.4 microcuries per mg. of phosphorus. This is only one-tenth of the specific activity in the $Na_2HP^*O_4$ used. This dilution can be accounted for by the presence of inactive phosphorus in the original seedling tobacco plants. That the radioactivity was associated with the virus molecule, was demonstrated by the fact that the specific activity remained constant through several recrystallizations.

Each of 105 mice were injected intravenously with 0.4 ml. of the crystalline radioactive TMV. This volume contained about 0.6 mg. of virus with 3600 counts per minute. Each of these 105 mice, plus 5 controls, had been given an initial injection of normal TMV 18 days previously. This procedure was followed because we wanted to be sure of having readily measurable amounts of circulating antibody after injecting the radioactive TMV. For the duration of the experiment, the mice were kept in metabolism cages, so that the feces and urine could be collected at various intervals for the determination of the amount of radioactivity excreted.

Twenty-four hours after the injection of the tagged virus and at intervals up to thirty days, groups of 5 mice were etherized and bled from the heart and the serum samples pooled for the titration of circulating anti-TMV. After bleeding the 5 mice, approximately 250 mg. slices of liver were removed from each mouse and the samples pooled. These were placed in a metal cup and dried at 125° C. for twenty-four hours. The dried samples were then weighed and put in a furnace and ashed at 600° C. for twenty-four hours. The ashed samples were used for the determination of the radioactivity. It was felt that ashing was necessary because of the variable amount of self-absorption observed when counts were made on the dried material. The whole spleens from the 5 mice and aliquots of the feces and urine, and in some cases the remainder of the carcass, were dried, weighed, and ashed in a manner similar to that described for the liver.

The radioactivity of the ashed samples of liver, spleen, feces, urine, and carcass was measured by means of a Geiger-Müller counter and a

mica window counter tube. The counting was done under rigidly standardized conditions, so that the day-to-day measurement of the radioactivity would be comparable. The radioactivity found in each tissue was expressed as counts per minute per milligram of tissue and was corrected for decay and self-absorption wherever necessary.

Autoradiographs were made of sagittal sections of mice, 24 hours after the intravenous injection of radioactive $Na_2HP^*O_4$ and tagged TMV, respectively. These autoradiographs were prepared by freezing a killed mouse in a dry ice acetone bath and then cutting a 3-4 mm. sagittal section with a band saw. The sections were then lyophilized and impregnated with paraffin, and 1 mm. sections were prepared from them with a microtome. The 1 mm. sections were placed in intimate contact with x-ray film and exposed for varying periods to obtain the autoradiographs.

Twenty-four hours after the injection of radio $Na_2HP^*O_4$, there was a very general distribution of radioactivity, so that the outline of the whole section was readily visible as well as cross-sections of various tissues such as the liver and the intestines. However, the greatest concentration of radioactivity was found in the bones as shown by the tooth, ribs, backbone, and pelvic bones. An area just under the intestines showing marked radioactivity was tumorous tissue.

In the case of the intravenous injection of tagged TMV, the auto-

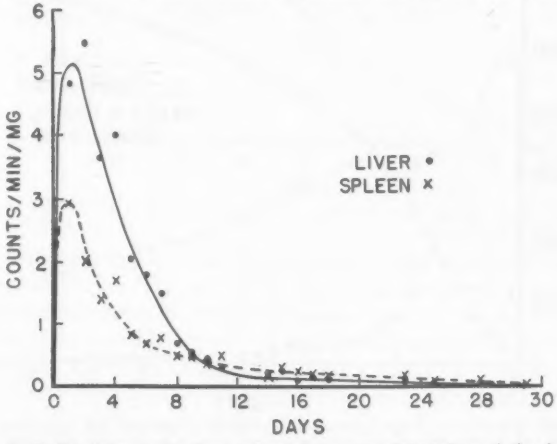

FIGURE 1. Radioactivity in the liver and spleen in counts/minute/mg. of the dry weight. (From J. Immunol. 55: 15-26. 1947.)

radiograph was, as would probably be expected, markedly different from $Na_2HP^*O_4$, since the only tissue readily observable was the liver.

FIGURE 1 shows the radioactivity in counts per minute per milligram that was found in the liver and spleen over a period of 30 days after the intravenous injection of tagged TMV. The highest counts were observed in the 24-hour samples, which, however, does not imply that this was the peak, since no determinations were made prior to this time. The radioactivity in both tissues dropped rapidly during the first 8 to 12 days, so that by the 14th day 95 per cent of the 24-hour activity was lost. During the last 2 weeks, *i.e.*, from the 14th day on, the rate at which the radioactivity disappears is much slower, so that only 56 per cent of the radioactivity observed on the 14th day is lost. The significance of the change in the rate at which the radioactivity is lost between these two periods will be discussed subsequently.

Before continuing, one interesting observation should be made. It has been generally reported that the liver is very effective in removing foreign proteins from the blood stream, but that per milligram of tissue the spleen removes greater amounts. This does not appear to be true for the mouse and intravenously injected TMV, since at 24 hours a milligram of liver contains almost twice as much radioactivity as a milligram of spleen.

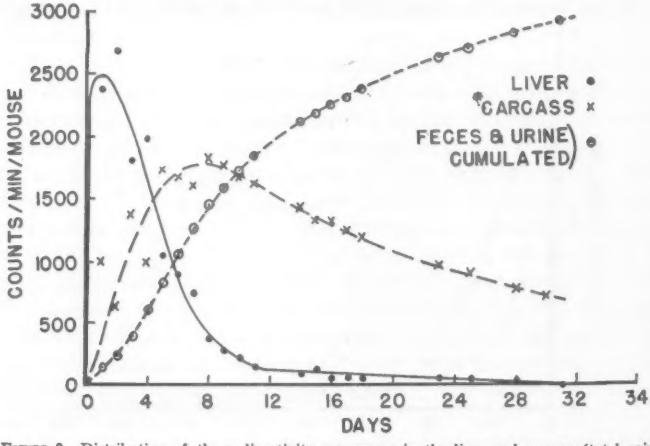

FIGURE 2. Distribution of the radioactivity per mouse in the liver and carcass (total animal minus liver and spleen), respectively, and the amount excreted in the feces and urine. (From J. Immunol. 55: 15-26. 1947.)

FIGURE 2 shows the radioactivity per mouse for the liver, for the carcass minus the liver and spleen, and the cumulated data for the radioactivity excreted in the feces and urine over a period of 30 days. Twenty-four hours after the injection of the tagged TMV, 63 per cent of the injected dose was found in the liver, 3 per cent in the spleen, 3 per cent in the feces and urine, and 30 per cent in the remainder of the carcass. As shown in FIGURES 1 and 2, the radioactivity in the liver drops off rapidly during the first 8 to 14 days, and then at a much slower rate during the remainder of the period observed. The radioactivity in the carcass (minus liver and spleen) increases up to a maximum on about the 8th day and then slowly decreases for the remainder of the period. Radioactive phosphorus is excreted at a fairly rapid rate during the first 12 days and at a slower rate for the remainder of the period.

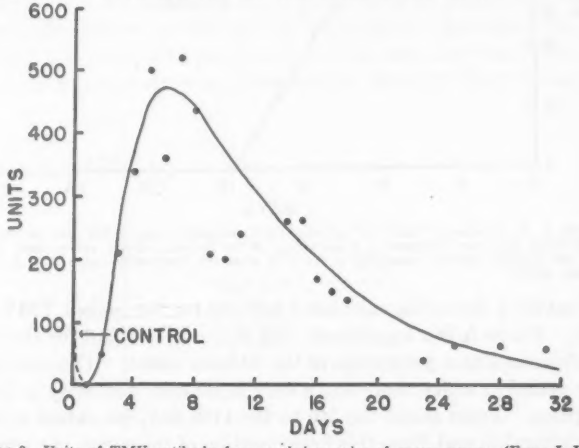

FIGURE 3. Units of TMV antibody from pooled samples of mouse serum. (From J. Immunol. 55: 15-26. 1947.)

FIGURE 3 shows the antibody produced in response to the injection of the tagged TMV. The amount of circulating antibody is expressed in terms of arbitrary units in which it is assumed that the antibody titer in the control mice equals 100 units. Twenty-four hours after the injection of the tagged TMV, no circulating antibody could be detected, while after 48 hours approximately 50 units was found. The antibody titer increased to a maximum of about 500 units between the 6th and 7th days. From this point on, the amount of circulating antibody decreased.

It is obvious that there was considerable fluctuation in the amount of circulating antibody, even though the units represent the values obtained on pooled serum samples from 5 mice. However, it is probable that the curve represents the relative day-to-day values.

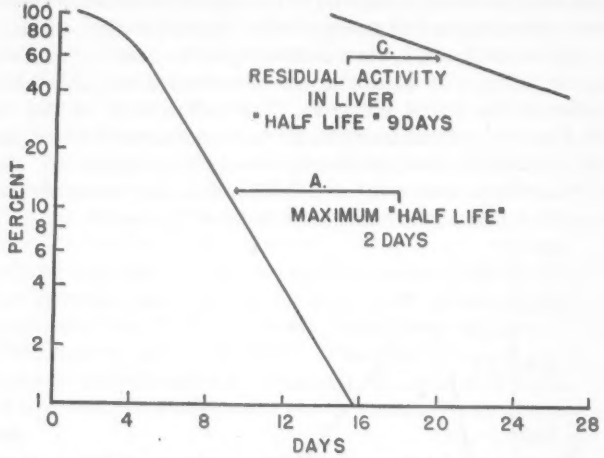

FIGURE 4. A, maximum "half-life" of the TMV calculated from a log plot of the residual radioactivity in the liver, expressed as a percentage of the 24-hour activity *versus* time. C, "half-life" of the residual activity remaining in the liver after the fourteenth day. (From J. Immunol. 55: 15-26. 1947.)

FIGURE 4 shows the calculated half-life for the tagged TMV in the mouse. Curve A is a logarithmic plot of the liver count for the first 11 days expressed as a percentage of the 24-hour count. This corresponds to the period in which there was a very rapid loss in activity in the liver and spleen. From about the 5th to the 11th day, we obtain a straight line relationship and, from this linear portion of the curve, one can calculate the maximal half-life of the TMV. What is meant by half-life in this instance is the time required for 50 per cent of the TMV to be broken down or destroyed. As indicated, the half-life is about 2 days. This calculated half-life of 2 days is a maximum value, since the radioactivity measured in the liver includes a certain amount of radiophosphorus that has been re-utilized by the mouse and incorporated into naturally occurring hepatic and splenic phosphorus compounds. That the mouse can and does re-utilize some of the phosphorus from the TMV, was demonstrated by obtaining lipid extracts of livers at various times

after the 14th day. The extracted lipid fraction contained from 10 to 20 per cent of the total radioactivity found in the liver. Since the TMV does not contain lipids, it is evident that the mouse has used some of the breakdown products for the synthesis of naturally occurring phosphorus compounds.

The residual radioactivity found in the liver and spleen after the 14th day is probably entirely due to re-utilized phosphorus and, as shown in curve C, it has an entirely different half-life, *i.e.*, 9 days instead of the 2 days calculated for the TMV. Curve C is a logarithmic plot of the liver count from the 14th to the 28th day, expressed as a percentage of the count on the 14th day. As one might expect, the 9-day half-life for the residual activity approximates closely the rate of excretion of radioactivity during this period.

Because of the re-utilization of phosphorus by the mouse, it is difficult to define the exact time for the complete breakdown of the TMV. However, it is possible to postulate from the data that more than 99 per cent of the injected virus had been broken down by the 14th day.

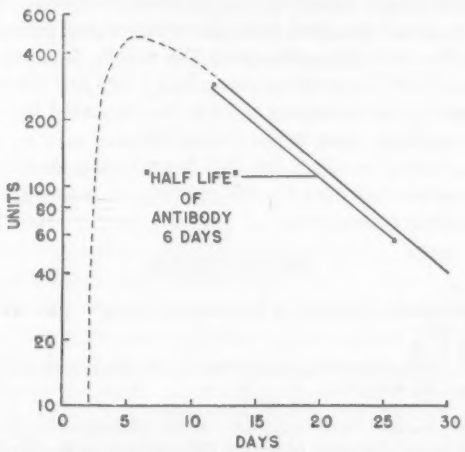

FIGURE 5. Half-life of the antibody molecule calculated from a log plot of the units against time. (From J. Immunol. 55: 15-26. 1947.)

FIGURE 5 shows the units of antibody plotted logarithmically against time. From the straight line portion of the plot, *i.e.*, from the 12th day on, one may calculate the half-life of the TMV as shown, which

is about 6 days. This 6-day half-life assumes that there is a negligible amount of antibody produced after the 12th day.

It should be noted that there is an approximate coincidence between the time of the probably complete breakdown of the TMV, *i.e.*, by the 14th day, and the linear decrease in circulating antibody from the 12th day on. This coincidence could be interpreted to indicate that antigen must be present in the antibody-forming tissues in order to have continuing antibody formation.

In conclusion, there are a number of things that should be pointed out. First, only the nucleic-acid portion of the TMV molecule was tagged and, therefore, we have no measure of the fate of the protein fraction. However, it is probable that the destruction of the TMV molecule would parallel the breakdown of the nucleic-acid portion of the molecule. Second, since the liver took up a major portion of the injected TMV, it is assumed that the fate of the virus in other tissues of the reticuloendothelial system parallels its fate in this organ. Some support is provided for this assumption in that the rate of disappearance of radioactivity in the spleen approximately parallels that found for the liver.

Third, it should be noted that Heidelberger[5] has reported a 14-day half-life for the antibody molecule in the rabbit. As previously indicated, the anti-TMV appears to have a 6 to 7 day half-life in the mouse. This difference in half-lives may possibly be accounted for by the difference in the metabolic rates in the two animals.

Finally, it may be concluded that tagged antigens offer a very real and a very interesting tool for the solution of some of the unsolved problems in immunochemistry.

REFERENCES

1. **Hevesey, G.**
1940. Application of radioactive indicators in biology. Ann. Rev. Biochem. **9**: 641–662.

2. **Hamilton, J. G.**
1941. The applications of radioactive tracers to biology and medicine. J. Appl. Phys. **12**: 440–460.

3. **Tuttle, L. W., L. A. Erf, & J. H. Lawrence**
1941. The phosporus and metabolism of the nucleoprotein, phospholipid and acid soluble fractions of normal and leukemic mice. II. J. Clin. Invest. **20**: 57–61.

4. **Marshak, A.**
1940. Uptake of radioactive phosphorus by nuclei of liver and tumors. Science **92**: 460–461.

5. **Schoenheimer, R., S. Ratner, D. Rittenberg, & M. Heidelberger**
1942. The interaction of antibody protein with dietary nitrogen in actively immunized animals. J. Biol. Chem. **144**: 545–554.

SECTION OF PSYCHOLOGY

April 21, 1947

Doctor Anne Roe, New York, N. Y.: *Personality and Vocation.*

Before beginning this discussion of why people take on the jobs they do and why they go about them the way they do, I should like, first, to ask the question, "Why do people work?" The obvious answer, that people work to earn a living, is by no means the whole story. Of course, it is a fact, but I should like to call to your attention the further fact that there are cultures in which the problem of earning a living is a very different matter and occupies much less of the individual's time and energy. In our culture, just "living" is a pretty complicated affair, which makes earning it more complicated, and this makes living more complicated, and so it goes on. How we ever got that way is a question I shall avoid discussing at the moment. Instead, let us consider some aspects of the present situation which have a bearing on the problem before us, and in order not to complicate the discussion unbearably, assume that we are considering so-called normal times with a good level of employment.

Let us note, first, the amount of time we spend working, which we take for granted. I suppose that most of us work at least seven or eight hours a day, at least five and maybe six or seven days a week at some defined job; that we spend about the same amount of time sleeping; and that everything else, eating, making love, spanking the children, playing bridge, going to the movies, cheering the Dodgers, has to come out of the rest of the 24 hours a day. It is this defined job, on which we spend so much time, with which we are concerned here.

This balance of work, sleep, and play must be satisfying to most of us (even discounting the powerful effect of having been brought up in the system), or there would be a lot more who attempt to flee to the South Seas or return to the land. And although we may day-dream of the pleasures of life with a minimum of occupation, like the little boy—

> "I wisht I was a little rock a-settin' on a hill
> A-doin' nothin' all day long but just a-settin' still
> I wouldn't eat, I wouldn't sleep, I wouldn't even wash
> But just sit down beneath a tree, and rest myself, by gosh . . . "—

I strongly suspect that it is just as well we do not get beyond the dream most of the time, or we should be afflicted with galloping boredom.

Now, boredom is an emotional state which people go to great lengths to avoid, and its cause seems to be quite simply absence of occupation. Why does this seem such an intolerable state? Lewinsky[1] remarks, "There seems to be an inner force which seeks outlet in regular and regulated occupation." She discusses relationships between sexuality, aggression, and occupation and points out that civilized society tends to keep everyone occupied and that refusal to work is taken as a sign of open aggression. To be without a job, whether due to one's own inadequacies or errors or not, is a difficult pill to swallow. Most studies of adjustment in old age point out the importance of meaningful occupation for the emotional health of the individual, emphasizing that it is often the lack of this that makes for so much unhappiness in later life. Moreover, as Kuhlen[2] points out, large numbers of adults are dissatisfied with their jobs even when the financial returns are adequate. Obviously, they had expected more of their work than pay.

It is clear, then, that there is more to working than earning a living. We should not be surprised that there is more to choosing a job than just finding one. Before discussing a theory of the nature of work and of the relationship of the individual to it, let us turn to such evidence of the nature of this relationship as is available.

An enormous amount of work has been done on the testing of aptitudes and special skills, and there has also been a great deal of work on vocational interests. However, both of these problems are only peripherally related to our present one. It is obvious that, if a given job requires a certain level of intelligence or certain special skills, any person lacking these will not perform well at the job. On the other hand, there is no guarantee that any person having them will therefore perform well at it. That the programs in industrial psychology have not gone farther than they have, seems to me to be largely because job allocation has been conceived in terms of aptitudes rather than in terms of the whole person who is to do the job. Vocational counselors themselves are realizing this more and more. Orrico[3] puts it that "Personality defects far outweigh aptitudes, however outstanding, in their influence on the success one will enjoy in a given occupation or profession." The importance of extreme variations is fairly obvious. You would not, for example, suggest that a man with claustrophobia should take a job as an elevator operator or coal miner. But phobias or obsessions are not necessarily generally disqualifying. The best housecleaning I have ever had done was the work of a man who was a severe obsessional neurotic—he happened to be obsessive about dirt.

Korner[4] has written a thoughtful report on the causes of inappropriate vocational choice, which recognizes the emotional meaning of work. But there have actually been very few studies investigating any relationship between personality and occupational choice and performance.

Vernon[5] has studied occupational choice and personality, concluding that, when the occupation is felt as an essential function of the personality, its choice is determined by various "drives" of personality, such as security, pleasure, activity, self-reliance, etc. This I am sure is true, but I should like to reduce the problem to somewhat simpler terms and shall attempt to do so later.

There have been studies of a few groups by means of personality inventories. These are pencil-and-paper tests, requiring the subject to answer a series of questions. Usually he answers yes or no to such questions as "Do you have headaches more than most?", etc.

Johnson[6] has reported that seminary students were entirely heterogeneous on this test, and that life insurance salesmen were homogeneous in only one respect—dominance. McCarthy[5] reported Catholic seminarians had more neurotic tendency, more self-consciousness, more unsatisfactory total adjustment, higher religious interests and were more submissive than other students of the same age.

Hampton[8] says that "the grocer has an average personality in no way distinguished from the large majority of people."

Dodge[9] found some traits common to successful clerical workers—they are not moody or subject to worry, are even-tempered, unwilling to accept responsibility, non-social, lack self-sufficiency, and do not crave admiration.

That nothing very profound has emerged from these studies is, in part at least, a reflection of the superficiality of the instruments employed. Recently, the Group Rorschach Method has been applied to a number of different vocational groups, chiefly to validate the method by comparison with results on individual tests. Except for a very few papers, usually with very few subjects, vocational analyses have not been made. Kaback,[11] however, tested 300 men (75 each accountants, accountancy students, pharmacists, and pharmacy students) and, after an elaborate analysis, found no clear-cut personality type in either vocation.

This brings us to my own studies on artists and scientists. They have been published separately, and any who are interested in the technical details will find them in those reports.[11] Here, I shall try to

pull out the highlights of each study, offer some comparison of them, and then against that background try to indicate the lines along which I think a theory of the relationship between occupation and personality can be developed.

The study of artists was undertaken originally for the Yale School of Studies on Alcohol, and was planned to investigate any relation between use of alcohol and artistic creation, but the material was so rich in other ways that it led me rather far afield from the original purpose. I have never undertaken any study which gave me such rich personal rewards. The artists cooperated so fully, were so extremely generous with their time and their interest that it was a profoundly moving experience to work with them. I came out of it with such strong personal reassurance of the essential decency of man that all the evidence to the contrary which the last few years have produced cannot shake me.

The twenty men who were the subjects of the first study are all artists of top rank. They represent most of the major trends in present-day painting. They averaged 51 years of age at the time of the study. The data consist of interviews, study of their paintings, critical opinion about them as expressed in various art journals, and of the results of two personality tests.

These two tests were the Rorschach and the Thematic Apperception Test. I will confine discussion tonight to the results on the Rorschach. This test consists of a series of 10 ink-blots, reproduced on cards for individual administration or thrown on a screen for group administration. The task of the subject is just to tell what he sees in the cards or what they look like to him. The responses are scored for such things as the proportionate use made of the whole blot or of large or small details of the white spaces, the clarity of the forms perceived, whether or not movement is seen in the cards and of what sort this is, and what use is made of color and shading.

From this, it is possible to infer a good many things about the structure of the subject's personality. If this seems a little esoteric, let me give a simple illustration, which should be qualified by the statement that, in this method, no factor is considered by itself but only against the background of the rest of the record. We have found that, in general, a subject who customarily uses the whole card in his response, either fitting details into this or disregarding them, is one who tends to more abstract thinking, whereas one who rarely uses the whole card but picks out large obvious details is one who is much more likely to concern him-

self with practical, concrete matters, and is not much given to abstracting and generalizing. The first one does not see the individual trees in the forest, while the other does not see the forest for the trees.

The artists showed extreme heterogeneity in the general personality structure presented, and, to my astonishment, many of them did not give any of what we have supposed to be indications of creative ability. Though some tendencies appeared in most of the group, these did not distinguish them from all other groups, with one possible exception. The artists were characterized by above-average intelligence, unusually great use of whole responses, marked prevalence of color and shading shock, and overproduction of responses of sexual and anatomical content. They gave very few Technical Responses, only half of them giving any of these. For this group, I considered as Technical Responses—a category which I use for responses referable to the specific profession of the subject—such as "It reminds me of a painting by Kandinsky." It is probable, however, that to have so classified the anatomical responses would be more nearly analogous to the bones I called Technical Responses for the scientists. It is these that distinguish this group.

The Rorschach has been considered to offer good indications of the presence or absence of "creative ability." About half of the group, however, did not give any striking indications of this, and for some of them the indications were quite to the contrary. Yet, these men are demonstrably artists, and successful ones at that. I do not think the difficulty is only a semantic one, which leaves us with two alternatives: (1) Creative ability may exist without being shown in the Rorschach, or we may recognize some indications of it and not others; or (2) One may be a successful artist in our society without having creative ability. I am not prepared to support either alternative at this point. To those who would greet the latter with loud cheers, however, I would utter the caution that, of the six men who gave none or only one human movement response, which is one of the indicators we use for this, only one is an "abstract" painter.

Analysis also showed that, as a group, they have a characteristic emotional adaptation which is non-aggressive and rather passive in nature and somewhat more feminine than masculine by our cultural stereotypes. Lest there by any misunderstanding, let me state that the problem is not one of homosexuality, which is not present in any of this group. It has been my experience that this type of adaptation is characteristic, in our society, of the sensitive, intelligent man who follows more or less intellectual pursuits.

Choice of this vocation, so far as I was able to learn, did not result solely from special abilities, nor primarily from a particular personality pattern, nor from any disparate elements in the personality so far as we are now able to isolate them. Study of the individual lives, however, makes it clear that this profession does satisfy emotional needs for most of the men which have not been satisfied in other ways, and gives us a clue not only to the meaning of creativeness, but to the meaning of all work activity. I shall defer discussion of this point for the time being.

A prolonged attempt to discover general relationships between any Rorschach determinants and the way in which the men painted was not very successful. I can, however, offer a few generalizations, though I do so with some hesitancy. The representational and naturalistic painters do seem to be better adjusted on the whole than the others. The men who are noted as colorists are more responsive to the color cards than the others. The use of color in painting does not bear any simple relationship to personality structure, but it does appear that the anxious, cautious, and compulsive person does not use color as freely or brilliantly, or for fun. There is an association between originality in painting and creative ability as spotted by the Rorschach, and there is also an association between originality in painting and the use of white space as a determinant on the Rorschach.

In individual instances, performance on the Rorschach was often extraordinarily illuminating in considering aspects of the painting. I can mention only a few of these. One painter stated that while he has no difficulty with the general plan of a painting, he always has trouble with some little detail. One of the most characteristic things about his Rorschach was that he constructed quite good wholes, but in a number of them some small detail was either unrelated or bizarre, and the nature of these gave some clue to the source of his troubles. Another painter commented that he was not constrained by the things he saw in front of him; his Rorschach amply confirmed this. The man who used the fewest whole responses often has difficulty with making his compositions a unified whole and is in general much less concerned with this than are other men. A painter whose protocol is full of references to terrifying things in empty space customarily covers his canvas with a mass of details, leaving no large undifferentiated surfaces.

The study of scientists is much less complete. Perhaps I should specify it as a study of vertebrate paleontologists, for I do not know how far vertebrate paleontologists are representative of scientists in general.

The material I have consists of Rorschach records taken, by the group method, of members of the Society of Vertebrate Paleontology. The test was first administered at the annual meeting of the society two years ago and, later, to members of the department in the American Museum of Natural History who happened not to be present at the meeting. In this report, I excluded the records of members not full-time workers in paleontology and also the records of the women, which are very unlike those of the men. This leaves eighteen in the group at the scientific level, and nine technicians, *i.e.*, field and laboratory men. The group, though small, is actually an excellent sample of the men in this field. Differences between these groups are not great and are chiefly reflections of differences in educational and intellectual level, except that the scientists give a great many more Technical Responses.

Unlike the painters, however, this group is surprisingly homogeneous in respect to certain personality characteristics. They tend strongly to abstractions and to formalized, objective thinking, and most of them show a marked inhibition of any tendencies to project themselves into a situation. They empathize little, relatively speaking, either with things or with other people. They tend to have a rather passive emotional adaptation, like that of the painters, although the evidence here is not so decisive. Very few of them manifest any creative capacity.

I have not studied the individual work of these men for comparison with personality characteristics. I do know that one of them, only five per cent of whose responses used the whole card, does beautiful detailed descriptive work, but never generalizes. I hope, some time, to check on my suspicion that the difference between lumpers and splitters in zoological classification is associated with a difference in personality in this respect! I can add, with the caution that the groups are very small, that those whose work is the most broadly theoretical and most widely significant are among the few who do not show a severe inhibition or lack of creative capacity.

This picture is in accord with other studies. Munroe[12] found this type of protocol characteristic of girls who choose scientific subjects in college, and stated that they tend to have higher non-verbal than verbal ability on intelligence tests. It is well known from college entrance examinations that arts students excel in tests using largely non-verbal abilities. It is known, too, that women more often excel in verbal and men in non-verbal abilities. This, then, is such evidence as is available. From now on, I am hypothesizing, not concluding. I have already made

the point that work has emotional importance for a man. It is rather curious that, in general, therapists have neglected this, Hendricks[13] being one of the few who have paid attention to it. He has been concerned to integrate what he perceives as the emotional importance of work into general analytic theory, and has proposed what he calls the *work principle*. He says, "Work is primarily motivated by the need for efficient use of the muscular and intellectual tools, regardless of what secondary needs a work performance may also satisfy." He considers it an expression of the "instinct to master" and notes that this is similar to what Angyal[14] calls the drive for autonomy.

Angyal postulates two major drives, defined as follows: Because of the *trend toward autonomy*,

> ". . . the person has a tendency to master the environment, and by conquest and achievement to impose his intrinsic determination upon a widening realm of events . . . In the trend toward increased autonomy the biologically chaotic items of the environment are fitted into the structure of the individual's life. In the homonomous tendency the person seeks to fit himself into larger organizations. The *trend toward homonomy* expresses the tendency of human beings to share and to participate in, to fit into and to conform with, superindividual categories such as the family, the social group, a meaningful world order, etc. Characteristic examples of the trend toward homonomy are social, religious, ethical and esthetic attitudes."

Accepting this, for the moment, as essentially correct, where does work fit into the picture? There is no question but that work, any work, offers autonomous satisfactions. It may be that some jobs or types of work offer no more than this, plus, of course, economic return, except incidentally. By that I mean that the fact of being with other people and working with them is incidental to the job itself and gives homonomous satisfactions. This is well epitomized in the cartoons about "They don't speak our language."

Other jobs, however, may be a source of homonomous satisfactions in themselves; they are jobs a man can put himself into. The extent to which this can be done varies from very little all the way to the peak which is reached at the height of creative activity. In creative activity, both drives are deeply satisfied, and its only rival in this respect is mature sexual activity. Furthermore, there seem to be differences in the way in which a man puts himself into his work. These are most easily differentiated, I think, in terms of what he empathizes with most readily, —whether with people, with things, or with ideas.

The fact that, in our society, work is a major source, if not the major source, of autonomous satisfactions, and that it may also be a major source of homonomous satisfactions, explains the dominance of voca-

tional activities in our lives. It explains many other things. Many men have taken refuge in their work in the face of the most acute problems in family or personal relations and have been sustained thereby. To be unhappy in the work and frustrated in it, or to be without it, are major traumatizing situations. I should like to remind you that Ginzberg[15] found work relief immensely superior to other forms.

It seems reasonable to assume that there are individual differences in the strength of these two drives, both absolutely and relatively. Some selection of occupation can be expected, then, in a very general way, in so far as those who have stronger homonomous needs will gravitate into occupations which will help to satisfy these.

It is necessary here, to insert a word about sex differences. I have previously maintained that there is a fairly consistent sex difference in the relative strength of the two drives. Whether this is biological or cultural, I am not prepared to argue. In any case, autonomy has a distinctly "masculine" tinge and homonomy a "feminine" one. To a considerable extent, the men who follow many of the learned professions, and the arts, have, by old stereotypes, a somewhat more feminine emotional adaptation. It is understandable, then, why they have chosen these vocations. It is just in these that they can satisfy their greater homonomous needs.

On this basis, one might ask why there are not more women than men in any of these vocations. The simple fact is that the biological role of women, and to some extent the social role, confused as I am told the latter is at the moment, offer a multiplicity of homonomous satisfactions which men do not have. Hence, it is the woman with a relatively larger "masculine" component, or, as we have been putting it, with relatively strong autonomous drives, who is the one who enters the arts and the professions.

I have no evidence for other than professional occupations, nor sufficient personal acquaintance in other circles to be able to discuss the situation in them. At the professional level, however, it seems to me that the relative strength of autonomous and homonomous drives enters into professional choice, something in this manner. Men whose autonomous drives are strong but not markedly so, whose intellectual make-up tends to the relatively greater dominance of non-verbal ability, and whose homonomous drives seem to find satisfaction in empathy with ideas, with symbols of things rather than with the things themselves, are men who are well suited to the pursuit of science and likely to be at-

tracted to it. In another group, where homonomous drives seem to be very much stronger, but in whom the major need is for empathy with the world outside, with things rather than with people, or with people as a special sort of thing—in this group, who need to incorporate the world into themselves and then to re-express it, we find the artists. In this same group with stronger homonomous drives, we also find a more apparently feminine emotional adaptation. In still another group, homonomous needs are very dominant, and their expression is through empathy with people. These are the men who are most feminine in their emotional adaptation and who are happiest in such professions as the ministry, teaching, social work, and medicine.

I wish to emphasize the point that this is, as yet, only a hypothesis, and that much more study is needed. I offer it only as a hypothesis now, but as one which I have found meaningful and which offers leads for further research.

BIBLIOGRAPHY

1. **Lewinsky, Hilde**
1943. Occupation and obsession. Brit. J. Med. Psychol. **19**: 388–393.
2. **Kuhlen, R. G.**
1945. Age differences in personality during adult years. Psychol. Bull. **42**: 333–358.
3. **Orrico, A. J.**
1944. Occupational neuroses. Occ. Therap. **23**: 74–78.
4. **Korner, Anneliese**
1946. Origin of impractical or unrealistic vocational goals. J. Consult. Psychol. **10**: 328–334.
5. **Vernon, M. D.**
1941. The relationship of occupation to personality. Brit. J. Psychol. **31**: 294–326.
6. **Johnson, E. H.**
1942. Personality and religious work. Am. J. Orthopsychiat. **12**: 317–324.
7. **McCarthy, T. J.**
1942. Personality traits of seminarians. Stud. in Psychol. & Psychiat. **5**(4).
8. **Hampton, P.**
1940. Is there a "grocer" personality? Occupations **19**: 184–187.
9. **Dodge, A. F.**
1940. What are the personality traits of the successful clerical workers? J. Appl. Psychol. **24**: 576–586.
10. **Kaback, G. R.**
1946. Vocational personalities. An application of the Rorschach Group Method. Teachers Coll. Contr. Ed. **924**.
11. **Roe, Anne**
1946a. The personality of artists. Ed. Psychol. Meas. **6**: 401–410.
1946b. Painting and personality. Rorsch. Res. Exch. **10**: 86–100.
1946c. Artists and their work. J. Person. **15**: 1–40.
1946d. A Rorschach study of a group of scientists and technicians. J. Consult. Psychol. **10**: 317–327.

12. **Munroe, Ruth**
1946. Rorschach findings on college students. J. Person. **10**: 301–316.
13. **Hendricks, Ives**
1943. Work and the pleasure principle. Psychoan. Quart. **12**: 311–329.
14. **Angyal, Andras**
1941. Foundations for a Science of Personality. The Commonwealth Fund. New York.
15. **Ginzberg, Eli**
1943. The Unemployed. Harper & Bros. New York.

SECTION OF ANTHROPOLOGY

APRIL 28, 1947

DOCTOR GEORGE F. CARTER, Department of Geography, Johns Hopkins University, Baltimore, Maryland: *The Distribution of Races of Maize among the Indians of the Mississippi Valley.*

The new discoveries concerning the history of corn or maize have made possible a clearer insight into the sequence and route of diffusion of maize. This is achieved by two means: by the recognition that there are races of maize, just as there are of man, and by the study of the presence of knobs on the chromosomes of maize. The method of establishing races is very comparable to that in man. Several critical, measurable values are selected, as, for example, row number, kernel shape, ear shape, stem type, and so forth. Knobs on the chromosomes of maize have proved to be of great utility. These markers are generally believed to be the result of the crossing of Andean maize with *Tripsacum*. In North America, most maize carries a considerable number of these markers, and the variation in number and their distribution on the various chromosomes have been found to have significant geographic distributions.

The material on which this study is based is the Department of Agriculture's collection of Indian maize. A chromosomal study of this material has been published by A. E. Longley.[1] In addition, I have taken 12 measurements on each of 350 ears from 27 Mississippi valley tribes. Both sets of data have been mapped. The resultant distributions are then interpreted in terms of the theory of age and area. The results indicate multiple introduction of differing races of maize into the United States. These introductions must have differed both in time and in route. The east of the United States differs in most characters from the southwest. Some southwestern tribes, however, share definite chromosomal characteristics with the east. Within the southwest, there are also recognizable differences. The Pima differ in many characters from the Pueblo. The Hopi and Zuni often differ from the other Pueblo groups. Other unusual relationships that were brought out are the resemblance of the Ute to the Northern Plains; the presence of two distinct strains of maize among the Pawnee, one characteristic of the Pueblos and the other

[1] **Longley, A. E.** Chromosomes of maize from North American Indians. J. Agric. Res. **56** (3): 177–196. 1938.

of the Plains; the peripheral and early character of the maize of mountain Cherokee; and other characteristics.

Attention is called to the utility of this means of working out relationships of peoples and of routes of diffusion. Anthropologists are urged to pay more attention to the cultivated crops that native peoples are growing.

SECTION OF PHYSICS AND CHEMISTRY

APRIL 25, 1947

MR. GILBERT D. MILES, Colgate-Palmolive-Peet Company, Jersey City, N. J.: *Interfacial Phenomena Associated with Mixtures of Surface Active Materials.*

The following is an outline of the various aspects of the problem which were discussed, together with the references to the literature from which the specific data were gathered for the presentation.

I. Minima in surface or interfacial tension concentration curves which are characteristic of mixtures of certain surface active materials.[1, 2]

II. The selective adsorption of certain components from solutions containing mixtures of surface active materials at air/liquid or benzene/liquid interfaces as indicated by foam or emulsion extraction.[2]

III. The influence of mixtures of surface active materials on the flow characteristics of bulk liquid through foam.[3]

IV. The effect of mixtures of surface active materials on foam stability.[4]

CONCLUSIONS

The surface or interfacial tension concentration curves for solutions of pure sodium alcohol sulfates exhibit minima when small amounts of the appropriate homologue are present.

The surface tension concentration curves for solutions of sodium alcohol sulfates exhibit minima when small amounts of certain water-insoluble surface active agents are added.

Foam or emulsion extraction experiments have demonstrated that the ratio of the concentrations of the surface active components in the surface compared to the solution, changes abruptly in the region of the minimum. This is in the direction of increasing concentration of the minor component in the interface at the minimum.

Small amounts of water-insoluble surface active materials decrease the rate of flow of liquid through foam compared to the solution without this addition, although the bulk viscosity may be only slightly different. There is a temperature for these systems above which the flow rate is not affected by the addition of the water-insoluble material.

In certain cases, solutions containing two surface active components exhibit foam stability of a higher order than is shown by solutions of either component alone.

Discussion

From the concept which was advanced elsewhere,[1] the surface properties of mixed alcohol sulfates can be explained as special cases of a "salt effect." It is conceivable that at least two types of mixtures may show no minimum at all:

(A) Mixtures of alcohol sulfates when the mole ratio of the components is high.

(B) Mixtures where the surface activity is nearly the same for each component alone.

It is also probable that the lowest surface tension obtainable with mixtures of alcohol sulfates will not fall below the lowest value obtainable with either component alone.

Aside from these points, it might be well to reconsider, in closing, the techniques themselves, and what part they have played in these studies of mixed surface active materials.

(1) Surface and interfacial tension measurements can, in some instances, be used as a sensitive test for the presence of traces of a second surface active material in a solution.

(2) Foam and emulsion extraction guided by surface tension measurements can be useful in the purification of surface active materials, and in measuring the surface concentration of one of the surface active components in a mixture.

(3) Data on the flow of bulk liquid through foam can be useful in gaining some insight into the flow characteristics of surface films of mixed as well as single surface active materials.

(4) Foam stability measurements are affected by surface phenomena which include properties not measured by the other three methods and, as such, they deserve to be included as part of such studies.

The use of these methods has provided information regarding the properties of mixtures of surface active materials which has been exceedingly useful in obtaining better insight into these phenomena.

Certainly, the theoretical significance previously attached to the appearance of minima in surface tension concentration curves must be

questioned in view of the fact that they may merely be a manifestation associated with systems containing more than one surface active component and, therefore, outside the scope of Gibbs' theorem.

REFERENCES

1. **Miles, G., & L. Shedlovsky**
1944. J. Phys. Chem. **48**: 57.
2. **Miles, G.**
1945. J. Phys. Chem. **49**: 71.
3. **Miles, G., L. Shedlovsky, & J. Ross**
1945. J. Phys. Chem. **49**: 93.
4. **Miles, G., & J. Ross**
1944. J Phys. Chem. **48**: 280.

MR. J. J. BIKERMAN, Merck and Company, Inc., Rahway, N. J.: *Tackiness and Adhesion.** (This lecture was illustrated by lantern slides.)

The interatomic forces known to us are definite functions of the interatomic distance. This means that if two neighboring rows of atoms in, *e.g.*, a metal wire or a glass rod are separated by force—that is, if the wire or rod is broken—and then brought in contact again, the attraction between them should be just as strong as before rupture. If this conclusion were confirmed by experience, there would have been no use for adhesives.

In reality, of course, the fragments of a broken wire or a broken rod do not stick to each other when brought together. This absence of sticking has two general reasons. (a) Every surface freshly formed in air immediately becomes coated with an adsorbed film of oxygen, water, etc. When the new surfaces are made to "touch" each other, the contact is established only between oxygen and oxygen, or between water and water, and, consequently, is readily broken. (b) All solid surfaces are rough and therefore can touch each other only in few discrete points, even if there is no gas adsorption (as in an exceptionally good vacuum).

Gas adsorption is a chemical, and surface roughness a physical phenomenon. The main purpose of this paper is to estimate the relative importance of chemical (or molecular) and physical (or rheological) factors in the action of adhesives.

Corresponding to the above causes of the absence of sticking, the adhesive must (a) displace the weak adsorption layers from the surface of the solids to be joined, and (b) fill in the depressions on both solid surfaces and form a continuous phase between them. Process (a) has a chemical, and process (b) a physical nature.

When the clearance between two solids is filled with a liquid adhesive, a greater effort must be expended for separating the solids than when there was adsorbed oxygen or water between them. This effort is called *tackiness*.

Tackiness is, in all probability, a purely rheological phenomenon. Such was the conclusion reached by J. Stefan as early as 1874. He measured the tension F(= force per area) required to separate two

* This address was delivered before the meeting of the Section of Physics and Chemistry on March 28, 1947.

plates jointed by a liquid adhesive, within t seconds, and found the equation

$$Ft = \frac{3}{4}\pi\eta a^2\left(\frac{1}{h_1^2} - \frac{1}{h_2^2}\right),$$

Here, η is the viscosity of the adhesive, a is the radius of the plates (or of the smaller plate if they are not equal), h_1 is the original distance of the plates and h_2 the distance reached after t seconds of application of the stress F. Usually, $h_2 \gg h_1$, and the term $\frac{1}{h_2^2}$ can be neglected in comparison with $\frac{1}{h_1^2}$.

Stefan's equation shows that there is no definite minimum force below which no separation takes place. If t is long enough, even a very small stress F will achieve separation. If molecular attraction had to be overcome before separation is possible, a minimum value of F would have been observed. Neither the properties of the solids nor the molecular forces between the solids and the adhesive affect the value of Ft. This value is determined by the geometry of the experiment (*i.e.*, the magnitudes a, h_1 and h_2) and by the viscosity of the liquid.

Stefan's equation has three interesting limitations:

(1) Its derivation assumes that the viscosity η is "normal," that is, that the adhesive is a Newtonian liquid. A corresponding equation applicable to "abnormal" liquids, and repetition of Stefan's experiments using "abnormal" liquids, would be welcome.

(2) By making t small, F can be made as large as desired. However, a large F can be greater than the tensile strength of one of the solids jointed. In this case, the solid breaks before the liquid has time to flow. This effect is easily realized by rapid peeling-off of a paper or cardboard strip recently glued to a solid.

(3) By making η very large, the product Ft can be made very large. A simple calculation shows that, in an apparatus of ordinary laboratory dimensions, many bitumen or rosin adhesives would give rise to products Ft so large that the joint would be stronger than copper or even steel for as much as several minutes. Here, again, the experience is quite different. Bitumen or rosin joints break long before the tensile strength of copper or steel is reached. The adhesive breaks as a brittle solid although, as experiments using small stresses prove, it is a Newtonian liquid.

A simple apparatus was constructed* to study the conditions which determine whether a uniform phase behaves as a solid or as a liquid. It is called *penetroviscometer*, and consists of a glass tube partly filled with adhesive and a cylindrical brass rod which can move within the tube along its axis. When the brass rod descends in the adhesive (because of gravitation), the rate of its movement is determined by the viscosity of the latter. This viscosity can be calculated from the rate of descent and proves to be independent of stress. When, subsequently, the tube is inverted so that the brass rod gradually slides out of the tube, the tackiness is measured (see the definition given above).

On the theory that tackiness is determined by viscosity, it is possible to compute the rate of sliding out. It proves to be smaller than the experimental rate. The cause of discrepancy can be seen. The rate of flow of the adhesive (upward, *i.e.*, toward the immersed end of the rod) is proportional to the ratio $(p_0 - p)/l$, p_0 being the atmospheric pressure, p the smaller pressure above the cylinder, and l the height of the column of the adhesive. Because of the irregularity of the adhesive-air interface, the height l is not constant along the circumference of the rod. The regions of a smaller l, that is, those above an indentation, show a greater rate of movement than other regions. This means that an indentation gets deeper, and its depth increases at an increasing rate. Finally, air reaches the upper end of the rod, and the rod falls out, when a large part of the adhesive is still included in the annular space between the tube and the rod. Apparently, a liquid behaves as a solid when disruptive forces applied to it are much greater than the forces of surface tension, gravitation, and similar factors which tend to preserve the uniformity of the phase.

The viscosity of the usual adhesives gradually increases after application, because of cooling, evaporation, imbibition or chemical reaction. When the viscosity reaches a nearly constant value, we say that the adhesive has set. The time interval between the application of an adhesive and the stabilization of its viscosity is termed *setting time*. During the setting time, many misfortunes can befall the adhesive film. If, however, no misfortune occurs, at the end of setting the adhesive forms a continuous solid bond between the members jointed.

When the effort required for rupturing the set joint, *i.e.*, the *final strength* of the joint, is measured, the result is different according to

*In the laboratories of The Printing and Allied Trades Research Association, London, England.

where the rupture takes place. One of the solids jointed can break: in this case, its tensile strength is the magnitude determined in the experiment. Or, the rupture can pass through one of the surface layers between the solids and the adhesive film: in this case, the experiments usually are poorly reproducible. Finally, the adhesive itself can give way. When this happens, the tensile strength of the adhesive film is the relevant magnitude. As a rule, the attractive forces between the molecules of the adhesive and those of the solids are not determined when the final strength is measured.

It was believed that, even when the rupture occurs wholly within the adhesive film, the final strength is different from the tensile strength of the adhesive, because experiments show this final strength usually to be greater than the tensile strength of a free specimen of an identical adhesive, and also to depend on the thickness of the film. However, the external differences between final strength and tensile strength are no sign of their different nature. These differences are due to the different dimensions of the test pieces. The rule, "the thinner the joint, the higher its strength," is a consequence of the other well-known rule stating that "the shorter the specimen, the greater its breaking load."

The higher breaking load of a shorter specimen is accounted for by the smaller probability of a very weak spot being present in a smaller volume. This probability theory of tensile strength can be proved in a simple way. Consider two identical specimens, 10 cm. long. One of these is tested directly, and the breaking load corresponding to 10 cm. length is determined. The other is cut into 10 segments, each 1 cm. long. Their testing yields 10 values of breaking load, of which 9 are greater than, and one is equal to, the breaking load of the long specimen, because one of the segments contained the weakest spot of the long test piece. This means that, on the average, the lowest breaking load observed in a batch of 10 segments of 1 cm. should be equal to the breaking load of 10 cm. long specimens. Unpublished measurements on glass filaments prove this theory in a quantitative way.

The above method was applied to paraffin wax joints between a steel rod and a brass plate. The joints were either 0.5 or 0.05 mm. thick (approximately). The lowest strengths in batches of 10 thin joints were somewhat greater than the strengths of 0.5 mm. joints, so that there was no quantitative agreement between theory and experiment. The small disagreement observed is attributed to the different conditions of setting (cooling) of the wax films of different thicknesses. The thicker the film,

the longer its cooling time and, presumably, the coarser the crystal grains in it. The effect of the setting conditions on the crystal size of the solidified film presumably accounts for the dependence of the final strength of a joint on the nature of the solids jointed. For instance, the rate of cooling would depend on the heat conductivity of these solids.*

*For a more detailed discussion of tackiness and adhesion, see J. Colloid Sci. 2: 163. 1947.

SECTION OF BIOLOGY

April 25 and 26, 1947

Conference on "*Allergy.*"

The Section of Biology held a Conference on "Allergy." Doctor Arthur F. Coca, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, April 25

Morning Session. "The Pharmacodynamics of Histamine," by Bradford N. Craver, Ciba Pharmaceutical Products, Inc., Summit, N. J.

"Experimental Anaphylaxis in Lower Animals: Fundamental Concepts of Specific Sensivity," by Beatrice Seegal, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Allergic Disease in Lower Animals: Classification of Allergic Disease," by Lester Reddin, Jr., Merck and Company, Rahway, N. J.

Afternoon Session. "Serum Sickness," by Samuel Karelitz, Mount Sinai Hospital, New York, N. Y.

"Contact Allergy (Skin)," by Max Grolnick, Jewish Hospital, Brooklyn, N. Y.

Saturday, April 26

Morning Session. "Atopic Allergy: Reaginic Sensitivity," by Matthew Walzer, Jewish Hospital, Brooklyn, N. Y.

"Allergy of Infection: Relation to Immunity," by Beatrice Seegal, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Allergy of Infection: Fungi and Fungous Products," by Marion B. Sulzberger, Skin and Cancer Unit, New York Post-Graduate Hospital, New York, N. Y.

Afternoon Session. "Familial Nonreaginic Allergy," by Milo G. Meyer, Michigan City, Indiana.

"Familial Nonreaginic Allergy as a Predisposing Cause of Common Cold," by Arthur P. Locke, Zonite Products, Inc., New Brunswick, N. J.

"The Antiallergic Action of Sympathectomy," by Arthur F. Coca, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

NEW MEMBERS

Elected April 24, 1947

LIFE MEMBERSHIP

Stewart, Alex, B.S., Non-ferrous Metallurgy, Pigments, Higher Polymer Chemistry. Director of Research, Research Laboratories, National Lead Company, Brooklyn, N. Y.

ACTIVE MEMBERSHIP

Adelberg, Edward A., B.S., Microbiology. Graduate Student, Yale University, New Haven, Conn.

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